Bozzetti et al. describe year-long, offline AMS measurements of filters collected in Marseille. The authors perform source apportionment analysis to the filters to demonstrate changing contributions of BBOA, OOA, HOA, and INDOA. The authors compare this analysis to previous studies (e.g. El Haddad et al. 2013) and winter-time measurements conducted using a high-resolution aerosol mass spectrometer. The authors find good agreement between the online and offline methods, and observe significant contributions from residential biomass burning during winter months. The authors provide additional analysis of the biomass burning factor and attribute changes in burning markers to differences in burning activities throughout the year. The authors also observe enhancements in methyl-nitrocatechol, which suggests secondary processing of the biomass burning emissions.

Overall, the paper is very well written, the methods are clear, and the interpretations of the data are reasonable. The PMF solutions, in particular, are incredibly detailed and thoroughly rationalized. The paper provides another example of the utility of off-line AMS analysis, which may serve as a useful low(er)-cost means for monitoring aerosol composition. While the study tends to confirm results previously observed in Marseille, it provides useful observations related to the seasonal changes in biomass burning markers. My biggest concerns relate to the over simplification of biomass burning sources, particularly to the assignment of periods described as lignin and cellulose burning. Upon addressing my comments, I recommend the manuscript for publication.

Major comments

Page 25, Lines 3 – 10: I'm persuaded by the argument that differences in biomass burning markers could be related to changing fuel types; however, I would recommend that the authors refrain from suggesting that the differences are strictly related to cellulose vs. lignin combustion. This also pertains to Fig. 11, which highlights periods of "lignin-combustion" and "cellulose-combustion." In reality, agricultural waste burning, open burning, prescribed burning, etc is the combustion of mixtures of lignin and cellulose-rich fuels; therefore, attributing changes in tracers to one plant structure or another downplays the complexity of biomass burning. I recommend the authors reframe the discussion to focus on changes in human activity, i.e. periods of increased prescribed burning, periods of increased residential heating, etc. Within that discussion, the authors may describe the differences in fuel composition, keeping in mind that mixed fuels (as well as burning conditions, fuel moisture content, etc) will contribute to the variability of biomass burning tracers.

Page 24, Lines 20-32: The authors discuss plant waxes, but do not provide any figures or correlations with other BBOA tracers. Are these supposed to be incorporated into Fig 11 (suggested at page 25, line 1)? I believe these traces need to be shown in the figure if they are to be discussed and attributed to different fuels

Page 10, line 4: The source apportionment performed on AMS data is very detailed; however, at times, I had difficulty following the progression due to the amount of detail provided. This discussion should be included in the main text, however I think it would be useful if the authors could provide a brief listing of the steps taken to perform this analysis at the beginning of this

section. For example, one could add "… In order to optimize the source separation, we performed sensitivity analyses on PMF solutions by (1) selecting number of factors, (2) constraining HOA and COA, (3) cluster anlaysis, (4)….". As the reader continues reading your method, they could reference this listing and follow the logical progression more easily.

Page 10, lines 24-26: For readers who may be unfamiliar with the a-value sensitivity analysis, it would be useful to explain here why one might apply this analysis to HOA and COA components. The authors mention that lack of acceptable tracers for COA emissions (line 31), but it may also be helpful to discuss that other studies have observed improved resolution of HOA after constraining these factors (e.g. Canonaco et al. (2013)), or that these two factors may exhibit similarities in the mass spectrum and/or diurnal profile, as demonstrated in the cluster analysis described in the SI.

Page 11, Lines 24 – 28 and Page SI 13, Lines 18-22: My understanding from reading this section is that the authors rejected clusters 4 and 5 primarily based on mass spectrum similarities with reference spectra, or by similarities with other factors (e.g. COA with HOA). From my untrained eye, it also appears that cluster 3 exhibits a strong correlation with cluster 4 (Fig S7 and Table S3, R = 0.93). Similarly, the correlation between Cluster 3 and NOx (R=0.57) is not substantially different from that of Cluster 4 and NOx (0.64). Would this also be grounds to reject cluster 3? Or, are the authors placing more weight on similarities mass spectra as opposed to similarities in temporal profiles? Personally, I believe similarities in mass spectra is a more important criterion, but other readers may disagree.

Page 14, lines 10-12: The authors indicate that a 5^{th} factor was resolved by source apportionment of the offline measurements, but not by online measurements. The authors note later in the manuscript (page 22, lines 4-5) that this was previously discussed, however I can't find this discussion in the PMF description. Please clarify.

Page 20, Lines 3-4: Since online and offline AMS measurements were not conducted simultaneously, I don't agree that you can make a direct comparison. Please revise.

Page 23, Lines 1-14. I'm confused about what message the authors are trying to convey with this discussion. Are the authors trying to attribute nitrocatechol formation to a chemical process, or is the focus to show that offline measurements can't capture the chemical evolution of these tracers due to their high reactivity and the low time resolution of offline analysis?

Page 24, Lines 7 – 17: The authors mention that the levoglucosan:nss-K ratio was 3.35 in winter at line 8, but then describe a minimum ratio in Jan/Feb of 6.3. I'm assuming this is a mistake, since I observe a minimum of ~3-4 from Fig. 11.

Figure 6: The x-axis is very difficult to read. The authors could remove the year from the dates, or average the collection interval to present a single value rather than a range.

Figure 10: I find this figure to be misleading. The authors note that the reader should only consider the monthly changes and not the day-to-day behavior since these measurements were not performed simultaneously; however, as a reader, my first intuitive response is to believe these measurements were conducted at the same time. Only after reading and interpreting the caption do I understand what the authors are conveying. In my opinion, the temporal profiles should be placed on separate axes or presented differently.

Figure 11: I find Figure 11 to be very difficult to read. The marker sizes are quite small, similar in shape (circles), and displayed on top of similarly colored, easter-egg-like backgrounds (lignin-combustion period, cellulose-combustion period). Personally, I think the figure is too busy and I struggled to immediately grasp its message. To simplify the figure, the authors could remove the measurements from 2011 and plot them separately in the supplemental information since these appear to be auxiliary evidence.

Figure 11: I'm confused how the authors derive the levoglucosan:vanillic acid ratio from the 2011 measurements. Are the authors using AMS tracers, or were filters collected? I'm assuming these represent AMS tracers since the scale is drastically different than that for 2012.

Minor Comments

Page 5, lines 6 -7: How frequently were the filters extracted from the sampler and stored? Weekly? Daily? I'm curious how long each sample was "aged" in the sampler prior to freezer storage.

Page 11, Line 2: I do not see a reference to Elser et al. 2015 in the bibliography.

Page 14, Line 3: Do the authors refer to the Pearson's R for COA, or its factor recovery?

Page14, lines 5-7: I believe the authors mixed up KOA and WSKOA?

Page 18, Lines 6-7, Page 23, Line 24, Fig. 11: At times, I'm confused as to whether the authors are referring to levoglucosan and vanillic acid measured by GC-MS or AMS. Can the authors please specify?

Editorial Comments

Page 20, Line 3: Add "of" between "one" and "the"